THE SIGNIFICANCE AND CONDITIONS OF RESEARCH*

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We welcome you tonight to the brotherhood of those men and women to whom scientific research is an absorbing occupation. You enter this group at a time which is unique in the history of the world—at a time when the scientific forces of the whole world are being organized for the prosecution of war. War is no longer merely the business of the army and navy; it is something in which the whole nation must take part and each of us must do his share. For the time being many must give their energies to the practical problems of defense and offense. One of our greatest American physicists has been working over a new range finder for the use of the navy and army. A great chemist has given months to the study of the best method of converting nitrogen into the nitric acid required for explosives. Others are at work on the production of optical glass for field glasses and other instruments. Statesmen have discovered that men skilled in research are indispensible for the successful prosecution of the war. They are looking now for immediate practical results, but the basis on which such results must rest is research work which has been continued for a century past. Just now we are calling loudly for inventions, and the war will doubtless bring many of these; but we, at least, must not forget, as too many do forget, that discoveries in science are of vastly greater consequence than invention. Faraday discovered electrical and magnetic induction. Without his discovery Morse and others could not have invented the telegraph. Scientific research is concerned primarily in making discoveries which will increase our knowledge. Invention takes discoveries and applies them to practical uses. You have been chosen for membership in Sigma Xi because we believe that you are capable of making discoveries.

It is worth our while, perhaps, to consider how discoveries are made. There is a popular notion that great discoveries are largely accidental. There is a certain amount of truth in this view. Newton saw an apple fall and was led to the discovery of the laws of

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gravitation. But countless numbers of men had seen objects fall before Newton. It was only because his mind had been prepared by years of study that his attention was fixed by the observation. In a similar manner if we cultivate the habit of thinking about scientific problems suggestions will come to us from time to time which it is worth our while to follow. Suggestions which are really valuable come rarely; when they do come they may furnish a basis for work which will continue for years or even for a lifetime. The best way to find such suggestions is, first of all, to put ourselves in contact with some of the greatest workers in our science, either as their pupils or by studying their published papers. There is a sort of heredity in science which is more important even than physical inheritance. Liebig worked with Gay Lussac, Kekulè worked with Liebig, Baeyer worked with Kekulè, and Professor Nef and Professor Stieglitz of Chicago worked with Baeyer. Students working with Professor Stieglitz are only the fourth generation from Liebig.

A second source for suggestions is contact with our colleagues in scientific meetings or through their publications. A third is the study of the foundations of science in the endeavor to impart them to others.

The next prerequisite for successful research is that we shall seize on suggestions which are really worth while. We sometimes hear expressions which seem to imply that it is worth while to gather a lot of observations without any hypothesis to guide us simply because the facts recorded may prove of value later. Such persons seem to be afraid that if they use a hypothesis to guide them they will be biased in their judgment. They forget that facts are usually of very little value until they are brought into relationships by some hypothesis or theory.

Sir J. J. Thompson has put this well in his discussion of the electron theory. He says, "The theory is not an ultimate one, its object is physical rather than metaphysical. From the point of view of the physicist, a theory of matter is a policy rather than a creed. Its object is to connect or coordinate apparently diverse phenomena and above all to suggest, stimulate, and direct experiments. It ought to furnish a compass which, if followed, will lead the explorer further and further into unexplored regions. Whether these regions will be barren or fertile, experience alone will decide; but, at any rate, one who is guided in this way will travel on in a definite direction and will not wander aimlessly to and fro."

After the suitable topic has been found, there comes, I think, the most severe test of whether an individual will continue in a career of productive research or not—the test of staying power. It is here that a large majority of our young men and women trained in research fail. The conditions under which a young man or woman who has recently taken a Master's degree or the degree of Doctor of Philosophy works are often not very conducive to research. There is often a heavy burden of teaching and it is usually easy to spend all of one's working hours upon routine duties. Only the man who has a genuine enthusiasm for new knowledge is likely to meet these difficulties and overcome them. He must have faith in himself and in his work and he must often be content for years with accomplishments which do not seem very great either to himself or to others. As this world is now constituted he cannot be very certain either of financial returns or of fame. He must find his joy in the work itself and must see clearly that the most valuable things in life are not dollars and cents and what they will bring, nor even fame and the recognition of others, but rather the consciousness of genuine work well done. There must be a devotion to one's work that is very closely akin to the spirit of the old prophets. I know that some will say that this sort of idealism is not to be found in practical America. I am sure that this is not true; we have it here in our midst and it is a spirit that is contagious and will grow, though genuine prophets are always too few.

Some things can be more easily understood from concrete examples and at the risk of seeming personal I will sketch briefly the careers of two such prophets who have developed here in our midst. Ten years ago a young man who had graduated at Worcester Polytechnic and who had spent one year in graduate work at the Massachusetts Institute of Technology came here as a research assistant. Three years later he took his Doctor's degree and became an instructor in our organic division. After three years of intensive work both in teaching and research, Professor Curtiss, with whom he had been associated, left us and he was placed in charge of the division. After three years more the man in charge of one of the largest firms manufacturing dyes in America selected Doctor Derick to organize the research laboratory for his factory, not because of any technical experience but because he knew how to apply the principles of physical chemistry to organic problems and

because he was recognized as one of perhaps a dozen leading workers in organic chemistry in America.

Professor Balke graduated at Oberlin College and studied for his Doctor's degree with Edgar F. Smith of the University of Pennsylvania. Professor Smith has been the best representative in this country of that good old-fashioned work in inorganic chemistry which has been less cultivated in the world of late than it deserves to be. Doctor Balke came here in 1907 to take charge of our instruction in general inorganic chemistry. He soon began research work upon the rare earths. In the course of seven or eight years there were only one or two other men working in this particular field who could be considered his equal in America. Two or three years ago he spent a comparatively short time in work on an industrial problem for the Pfahnstiel Company of Chicago and was rather phenomenally successful. Last summer he was asked by that company to organize a research laboratory for the study of the application of rare elements to industrial uses. That laboratory is now actively at work on problems directly connected with the national defense and is taking two other men from us this summer.

These two men found the opportunity for research right here among us. It became necessary last summer to search the country over for men to fill their places and we discovered that the number of young men who have taken their degrees during the last ten years and who have devoted themselves steadily to research is very small. One of the positions left vacant has not been filled because we have not been able to find a man whom we considered altogether suitable to fill it. I cannot believe that there are not in this country a considerable number of men who have the natural ability to fill these positions if only they had been willing to pay the price—enthusiastic desire for new knowledge, steady, persistent work in the chosen field, and a willingness to forego many things which are desirable enough in themselves but which are impossible if genuine, high scholarship is to be attained. And so the word which I wish to say to you is: The next five or ten years of your life are the golden years. Do not feel that you can let one or two or three of them pass without beginning independent research of your own. If you do, the chances are very strong that, while I am sure that you will all do good work and work well worth doing, you will fail of the highest ideals for a scientific career.